

FEW-BODY PHYSICS – THEN AND NOW

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ABSTRACT

A summary of the XIVth International Conference on Few-body Problems In Physics is given, with an emphasis on the important problems solved recently and the prognosis for the future of the field. Personal remarks and “homework” problem assignments are made.

INTRODUCTION

It is within the purview of a summary speaker (or perhaps it is even an obligation) to reminisce about the past, and I will do so. As a graduate student in the middle sixties, my activities were primarily directed at nuclear structure and electromagnetic interactions. Nuclear structure calculations deal with degrees of freedom in an economical way, ignoring those that don’t actively participate. When I finished my thesis and went to CERN, I discovered a completely different side to nuclear physics. At that time the field that we now call medium-energy physics was forming. It was largely an amalgam of electron scattering, high-energy (for that time) proton-nucleus scattering, and meson-nucleus interactions; I found the mix fascinating (and still do). I soon discovered a set of summer school lectures by Colin Wilkin[1], which played a very large role in my intellectual development. These pedagogical lectures treated proton-nucleus scattering in the Glauber formalism. To my delight all of the coordinates of the nucleus entered into his treatment, which included momentum conservation and redundant coordinates, in a completely microscopic approach. My first paper as a fresh Ph.D. used information from elastic electron scattering by ^4He to constrain the latter’s wave function, and then applied this to $p\text{-}^4\text{He}$ scattering. There were technical problems with this work and it was never published, but I applied the same ideas to the Coulomb

energy of ${}^3\text{He}$, and the resulting “hyperspherical” approximation[2] (derived independently by Fabre de la Ripelle) is both accurate and has withstood the test of time.

As a theorist, the beauty of this corner of nuclear physics lay in my control over all aspects of a system: the coordinates, spins, and isospins. Although calculating ${}^{208}\text{Pb}$ is no different in principle from the triton, it is currently necessary to restrict ourselves to a small (tractable) number of these coordinates. Then and now this restriction provides an operational definition of the few-nucleon systems: those that we can accurately calculate.

The first few-body conference that I attended was the 1974 meeting at Laval University in Québec City[3]. In my naiveté, I was both delighted and disappointed. People were actually trying to calculate the wave functions of three- and four-nucleon systems from first principles, although it appeared that they weren’t doing a very good job of it. Little did I realize how complicated the problem was, nor that we had years to wait before computational facilities became adequate to the scope of the problem. Nevertheless, it was this meeting that convinced me that accurate calculations and their comparison with accurate experiments are the keys to the success of our field.

In what follows I will comment on a few selected topics (concentrating on interdisciplinary aspects) and try to contrast these observations with similar material presented at Laval. Although most of the titles of the talks presented there would not be out of place in this meeting, the level of sophistication has increased greatly, and in some cases has a different qualitative scale. I will also list in the form of “bullets” those calculations and experiments that I believe need to be performed for orderly progress in the field.

Finally, at the end I will discuss a new approach that has already provided insight into the nuclear force, and that might give much more. This is the speculative part of the talk. It will be interesting to reread this in a few years!

THE FEW-NUCLEON SYSTEMS

Traditionally, the 2-, 3-, and 4-nucleon systems comprised the few-nucleon systems (FNS). What makes their treatment so difficult (compared to the atomic analogues) is the almost pathological nature of the nucleon-nucleon (NN) force. Not only are there very strong short-range repulsive forces, but the most important part of the potential is the tensor force, which mixes orbital and spin angular momentum. In addition, there are many (spin and

isospin) components. The recently developed Argonne V_{18} force has 18 separate components, for example. Because the Coulomb force is much simpler than the nuclear one, our colleagues in atomic physics have achieved a level of sophistication in calculations (which is exceeded only by the precision of their experiments!) that truly boggles the mind.

A certain class of few-nucleon calculations is often called “exact” or “complete”. My definition of such a class is that they solve the Schrödinger equation for realistic nucleon-nucleon potentials with an error of less than 1%. This was first achieved a decade ago for the triton and we have added the α -particle (^4He), the n - α resonances of ^5He and, recently, ^6Li . These systems in my opinion correspond to the current definition of the few-nucleon systems. Table I shows the unpublished Green’s Function Monte Carlo (GFMC) results of Brian Pudliner[4] for ^3H , ^3He , ^4He , and ^6Li using the Argonne V_{18} potential and the Urbana model-9 three-nucleon force adjusted to reproduce the triton binding energy. Note that the theoretical results are the ones with error bars! Clearly, there is no need for strong four-nucleon forces.

Table I

	<u>^2H</u>	<u>^3H</u>	<u>^3He</u>	<u>^4He</u>	<u>^6Li</u>
GFMC (MeV)	2.22	8.47(2)	7.70(2)	28.35(8)	31.5(4)
Expt. (MeV)	2.22	8.48	7.72	28.30	32.0

An interesting program of variational calculations of light nuclei by the Urbana-Argonne group[5] has made substantial progress in microscopic calculations of ^{16}O and ^{40}Ca . This technique makes use of a cluster expansion, which appears to converge rather well, at least for ^{16}O . The binding energy/nucleon that results is about 8 MeV, in good agreement with the experimental value, for a potential that includes both the Argonne V_{18} NN force and a three-nucleon force (3NF) that has been adjusted to fit ^3He . It would be very exciting if we could add all nuclei through ^{40}Ca to our list. Although variational calculations have greatly improved in recent years,

- a marriage of GFMC and cluster techniques (if possible) would be a great advance for our field.

SOME RECENT PROGRESS

Looking back 20 years, I marvel that any theoretical progress was made using the computational facilities that were available at that time. Calculations typically produced a few numbers, which were difficult to place in any context. As the facilities have improved, we are now able to generate a flood of numbers, which can be analyzed to provide insight into few-nucleon physics. My favorite numerical methods text is Richard Hamming's "Numerical Methods for Scientists and Engineers" [6], which has the following epigraph: "The purpose of computing is insight, not numbers." This clear and simple directive is no surprise to a physicist, but without lots of numbers we can have little insight.

As an example of this, a "scaling" plot [7] of the charge radii of ${}^3\text{H}$ and ${}^3\text{He}$ is shown in Fig. 1. Each symbol without an error bar is a calculation.

FIG. 1. Scaling plot for rms charge radii of ${}^3\text{H}$ and ${}^3\text{He}$ vs. binding energy.

The abscissa is the binding energy (E_B) of a particular nuclear force model, which includes a Coulomb interaction for ${}^3\text{He}$, and in some cases a 3NF. The ordinate is the corresponding charge radius. We see from this plot that a realistic (i.e., physical) value for the latter can be obtained only if the fitted curve is evaluated at the physical binding energy. The average behavior of the radius for ${}^3\text{He}$ and ${}^3\text{H}$ can be shown empirically to scale like $1/E_B^{\frac{1}{2}}$, which follows immediately from the asymptotic behavior of the wave function ($\sim e^{-\kappa\rho}$ with $\kappa \sim E_B^{\frac{1}{2}}$). We note that the divergence of the mean-square radius with vanishing binding energy is very similar to the divergences with vanishing pion mass that arise in chiral perturbation theory (χPT) calculations[8] of the pion or nucleon charge (or magnetic) radii. In the former case, the nuclear center-of-mass (CM) is surrounded by a “cloud” of nucleons, while in the latter case it is a cloud of pions. The difference between the ${}^3\text{He}$ and ${}^3\text{H}$ charge radii is due to the weaker force between two protons (in ${}^3\text{He}$) or two neutrons (in ${}^3\text{H}$), and that between a neutron and a proton. This causes the two protons in ${}^3\text{He}$ to lie further from the CM than the neutron and conversely for the proton in ${}^3\text{H}$. Since the charge radius is defined as the average distance of the charged particles (i.e., protons) from the nuclear CM, the charge radius of ${}^3\text{He}$ is greater than that of ${}^3\text{H}$.

This geometric effect is therefore the result of the inequality of forces between nucleons and is sometimes attributed to $\text{SU}(4)$ symmetry breaking or the S' -state, which are different names for the same physics[9]. My point is that if one had calculated only a few numbers and had been unaware of the dependence of many observables on E_B , there could have been a serious disagreement between theory and experiment. In some capture reactions scaling as severe as E_B^{-2} has been observed[7]. Finally, we note that the same mechanism leads to depolarization of the neutron in a polarized ${}^3\text{He}$ target[9], and to a nonvanishing neutron charge form factor in the naive nonrelativistic quark model[10].

Since this conference is a joint enterprise of nuclear and atomic physics, I would like to comment on some very interesting recent experimental work on the charge radii of the FNS. A contribution by the Yale group[11] illustrated how the isotope shift between transitions in ${}^3\text{He}$ and ${}^4\text{He}$ can be used to determine the difference in rms radii of these systems. Using the accurately known ${}^4\text{He}$ result, the value for ${}^3\text{He}$ is 1.9500(14) fm. This number contains a lot of physics that we would like to test. In order for an electric field to probe a nucleus, it must first “grab” the charged particles (the nucleons) before shak-

ing the nucleus. The nucleons themselves have an intrinsic charge distribution that is folded with that of the wave function. We will ignore other mechanisms, such as pion clouds, which can also affect the nuclear size. Removing the intrinsic nucleon radii leads[7] to

$$\langle r^2 \rangle_{\text{wfn}}^{\frac{1}{2}} = 1.765(6) \text{ fm} \quad (\text{expt.}) , \quad (1a)$$

while extrapolating the upper (fitted) curve in Fig. 1 to $E_B(^3\text{He}) = 7.72 \text{ MeV}$ gives

$$\langle r^2 \rangle_{\text{wfn}}^{\frac{1}{2}} = 1.769(5) \text{ fm} \quad (\text{theory}) , \quad (1b)$$

where the theoretical “error” is a subjective estimate of the fluctuations about the fit to the various calculations. The two experimental points in Fig. 1 were obtained from electron scattering, and the error on the ^3He result is seven times that of the Yale measurement. The agreement in Eqns. (1) is excellent, but this highlights a long-standing problem with the deuteron.

The deuteron rms radius extracted[12] from electron scattering data is

$$\langle r^2 \rangle_{\text{wfn}}^{\frac{1}{2}} = 1.953(3) \text{ fm} \quad (\text{expt.}) , \quad (2a)$$

while the recently constructed Nijmegen potentials[13] produce

$$\langle r^2 \rangle_{\text{wfn}}^{\frac{1}{2}} = 1.968(1) \text{ fm} \quad (\text{theory}) . \quad (2b)$$

Because many deuteron properties are determined primarily by its small binding energy, and to a lesser extent by OPEP, deviations of even 1% may point to a breakdown of the impulse approximation, although calculations suggest otherwise[12]. Atomic physics may have again come to the rescue. A recent determination of the isotope shift between hydrogen and deuterium in the 1S-2S transition energy can be interpreted as a measurement of the deuteron radius if other effects are removed. This leads to[14]

$$\langle r^2 \rangle_{\text{wfn}}^{\frac{1}{2}} = 1.973(7) \text{ fm} \quad (\text{expt.}) , \quad (2c)$$

which is in much better agreement with the best theoretical value. More accurate experiments are under way.

Finally, Fig. 1 shows that the tritium radius is not well determined.

- An accurate atomic measurement of the tritium-hydrogen isotope shift would be extremely valuable.

Two years ago I was asked to assess the status of theoretical calculations in the three-nucleon systems[15]. My criterion was whether or not complete calculations had been performed. This typically requires that all NN potential partial waves up to and including $J = 3$ be kept. It is convenient to break the 3N problem into four energy regions: (1) bound states; (2) zero-energy scattering; (3) scattering below the threshold (E_{th}) for deuteron breakup in N-d scattering; and (4) scattering above that threshold. I further divided these problems into (a) incorporating NN forces only; (b) including a 3NF; and (c) including a Coulomb interaction in the ^3He (or p-d) case. Based on these categories, I constructed Table II.

Table II

	<u>NN</u>	<u>3NF</u>	<u>C</u>
$E = -E_B$	×	×	×
$E = 0$	×	×	×
$E < E_{th}$	●	●	●
$E > E_{th}$	×	●	—

A “×” indicates that a complete calculation existed then. A “●” indicates that such a calculation now exists (or is underway) but didn’t then, while a “—” indicates that no calculation exists. A single Coulomb calculation by the Mainz group[16] using a limited number of partial waves has been performed, and this calculation shows significant ($\sim 20\%$) Coulomb corrections at modest energies, warning us that comparing p-d data with n-d calculations can be dangerous! Most of the entries in this table have occurred since 1990.

- A major goal for the field should be complete p-d calculations above breakup threshold with the Coulomb interaction included.

SCATTERING CALCULATIONS AND EXPERIMENTS

Scattering calculations (elastic and breakup) are very difficult, much more so than bound-state calculations. There were few contributions at Laval treating this topic. This has been the area with the greatest improvement, and one

where theory is beginning to play a vital (and historically unusual) role. Because complete three-body scattering calculations are now tractable (indeed, they are approaching maturity), theorists can make absolute predictions, which experimentalists love to test. Failures mean that the dynamics is inadequate and requires better forces. It may be possible to “see” three-nucleon forces in scattering reactions, and a large experimental effort has been mounted to look for these effects. Most calculations agree with experiments very well without 3NF; only a few disagree[17]. A very good sign from recent calculations is that some scattering observables appear to be fairly sensitive to these forces, although no definitive conclusions have been reached. Perhaps by the next meeting we will have a “smoking gun”.

I have been asked by a number of atomic colleagues why we place so much emphasis on the Faddeev approach in our calculations, when this is not necessary in atomic physics. The answer is “boundary conditions”. In situations where the boundary conditions are self-evident, such as bound states or scattering below the (energy) threshold for breakup (ejection of a previously bound particle), any technique works well. The extremely successful GFMC technique is based on the Schrödinger, rather than the Faddeev, equation. Above breakup threshold the boundary conditions are complicated and the Faddeev decomposition, which leads to the Faddeev equations, is very effective in implementing them. Other techniques exist, but have been little used in nuclear physics. The Faddeev decomposition has been so successful as a numerical procedure that it is also used for systems such as bound states where the boundary conditions are obvious.

One of the biggest advances in our field has been the increasing use of polarized beams and targets by experimentalists. The complexity of the nuclear force requires detailed polarization information in order to disentangle the dynamics. Comparison of the contributions at Laval with those here highlights this enormous improvement in capability. I would guess that the majority of the few-nucleon experiments reported here involved polarization. For one recent example of how far this can be pushed, I refer the audience to the Wisconsin[18] p-d experiments at 3.0 MeV (lab). With minimal assumptions two dozen phase shifts and mixing parameters were determined. One of my biggest (and most pleasant) recent surprises has been the successful implementation of the hyperspherical-harmonic-expansion method for nuclei by the Pisa group[19]. By incorporating explicitly the short-range correlations and using brute-force numerical techniques this group has performed complete calculations of bound states, zero-energy scattering (both n-d and p-d, with and

without a Coulomb force or a 3NF), and scattering below breakup threshold. The bound-state results agree with many other calculations, while the zero-energy scattering agrees with our own calculations[15]. I can personally attest that the latter were extremely difficult, so I am delighted that they have been confirmed. The Pisa results at finite energies appear to be in rather good agreement with the Wisconsin analysis, except for partial waves sensitive to a 3NF, which can make an appreciable difference. Coulomb effects were also shown to be important. I hope that these groups get together and combine their talents. Other experiments at very low energies are eagerly awaited from TUNL.

Repeating my earlier comment, implementing complete Coulomb calculations for p-d scattering above breakup threshold remains a priority for the field. There are many very precise p-d experiments, which require complete Coulomb calculations for their analysis. The recent progress in this direction by the Mainz group shows a substantial Coulomb effect and significant differences between p-d and n-d scattering at modest energies. Although these very difficult calculations are not complete, they point to the danger of comparing p-d data with n-d calculations.

RELATIVITY AND NUCLEAR FORCES

There were few contributions at Laval treating this subject, but many here. This is a somewhat controversial subject, but I will try to summarize what has been done. Naive estimates of $(v/c)^2$ in FNS can be obtained by using the uncertainty principle. We have $(v/c)^2 \sim (\bar{p}/Mc)^2 \sim (\frac{\hbar c}{Mc^2 R})^2 \sim 1 - 2\%$, for radii, $R \sim 1.5 - 2.0$ fm. What this argument fails to account for is that relativity introduces operators that depend on the nucleon momenta. Expectation values of these operators probe the tails of momentum distributions and are almost always larger than the estimate above. A number of calculations find that corrections to the kinetic and potential energies are roughly 5%, and because the NN system must be reproduced these cancel to a large extent, changing the triton binding energy by roughly 5% . It is a measure of our recent success that we now need to worry about effects of this size.

About a year ago, I collaborated with the Nijmegen and Iowa groups[13] to benchmark the triton binding energy for local potentials. The Nijmegen group have been engaged in a sophisticated and extensive partial-wave analysis of NN data. They constructed a number of NN forces (updated Argonne, RSC,

and several varieties of Nijmegen potentials) in various versions. Collecting all of these, one finds that the local ones produced triton binding energies of 7.62(3) MeV, while momentum dependence of a particular type (from relativity) produced additional binding (~ 100 keV). Other known relativistic corrections have not been included in most potentials.

The momentum-space Bonn B potential does include such effects in part. The Bonn potentials have always produced more triton binding than any other potential, and the reason for this has been something of a mystery. Part of the reason is that the 1S_0 potential is a bit too strong since it was fit to the n-p phase shifts. Correcting for this, the triton binding is still roughly 300 keV higher than the local potential result. In contributions to this conference[20,21], it was shown that the choice of π -N coupling in the Bonn potential leads to a nonlocal OPEP (i.e., tensor force) of relativistic origin, and this is presumably the reason for the difference. I used the word “choice” because chiral-symmetry techniques show that other forms are equally valid, and this choice is just one of many valid off-shell extensions of OPEP. I view this improved understanding as a major success of this meeting.

We also need to ask whether it is really necessary to define relativistic “corrections”. Why not simply use fully relativistic kinematics and forces? In principle the latter is the better approach, but current practice dictates the former approach in most cases. We still do not have fully satisfactory relativistic three-nucleon calculations. Calculations of this type are underway[22], but are not yet available. For more massive nuclei they are currently out of the question. Semi-relativistic calculations (employing $\sqrt{p^2 + m^2}$ for the kinetic energy of a nucleon) are feasible, however, for such systems by using Carlson’s clever trick[23] for implementing the relativistic kinetic energy in configuration space. Hopefully, modifications of the potential (at least to order $(v/c)^2$) can also be added to this procedure.

- Fully relativistic three-nucleon calculations will be required in order to benchmark the reliability of semirelativistic calculations, and as an accomplishment in their own right.

There is another reason to follow this path. Potentials have recently been designed that provide an excellent fit to the NN data[13]. Triton calculations seem to be indifferent to the details of the local potentials. On the other hand, arbitrary (weak) nonlocalities can be added to the potentials, which can still fit the data equally well. We cannot use the NN system to determine nonlo-

calities; we must use theoretical guidance, which highlights an old problem: we have a fundamental understanding of only a part of the nuclear force.

Fortunately, recent developments have eased this problem a bit. The Nijmegen group[24] have experimentally verified the presence of OPEP in the NN force. They fit the mass of the exchanged pions and find 139.4(10) MeV for the charged pions and 135.6(13) MeV for the neutral ones, in excellent agreement with the free masses. The tiny error bars illustrate the importance of OPEP. Indeed, OPEP produces about 70-80% of the triton potential energy[13]. Given this dominance, an obvious (and hopefully adequate) improvement to NN potentials would be to include relativistic corrections to OPEP and TPEP (two-pion exchange), and these should be both well-founded and largest. Theorists in the past had relatively few potential models to work with, and most of these had known (and often irritating) defects. The more well-crafted models that we have for our use, the better. We need to use potential models, not be their prisoner.

- The development of semirelativistic potential models is needed.

Ideally I would like to see an “error bar” of uncertainty established, which is a measure of our ignorance for the FNS. For example, is it possible to construct potentials with sufficient theoretical certainty that the uncertainty in the triton binding energy (ΔE) is constrained (e.g., $\Delta E \leq 0.25$ MeV)? This would then translate into an uncertainty for the 3NF. Even though such a limit is probably the best we can do (without directly solving QCD), it would be a wonderful accomplishment.

Finally, a comment by Nathan Isgur[25] in his talk reminded me of an obscure QED theorem, which in some sense explains why the hydrogen atom is so simple if one doesn’t look at the fine details. Nathan stated that the few-body problem is scale dependent. That is, a change in the length or mass scale changes the number of degrees of freedom (d.o.f.), or bodies, and hence this conference depends on a choice of scale! He remarked parenthetically that even the hydrogen atom is a many-body problem. Why? QED mandates that photons be exchanged between the electron and the proton in all possible numbers and orderings. If one were to take a snapshot of the atom at a particular time, there would be photons in the air, making the atom a complicated many-body problem involving the (virtual) photon “glue”. Fortunately (or even obviously), it was shown many years ago[26] that as one particle (the proton) becomes very heavy, the electron acts as if it were moving in a static

Coulomb potential. In other words, the “glue” decreases as the $(v/c)^2$ of the heavy particle decreases, and the problem becomes more potential-like. The same thing can be expected to happen in a nucleus: the more nonrelativistic the nucleus, the more potential-like the dynamics becomes.

QCD AND ALL THAT

I became a physicist during a time when we had little idea about the underpinnings of the strong interactions. This was the era of “you cannot use perturbation theory because the couplings are too large”, or “the model uncertainties are too large”. During the sixties one important event changed things forever: the discovery of chiral symmetry.

Chiral symmetry resolved a major nuclear physics practical problem, while providing explanations for other phenomena. The Goldstone mode realization of (broken) chiral symmetry makes the pion nearly massless on the hadronic scale, and allows pion exchange to dominate the nuclear force under ordinary conditions. The major practical problem that was resolved was the necessity of *ad hoc* “Z-graph” or “pair” suppression in relativistic treatments of the nuclear force. The off-shell pion-nucleon interaction can be incredibly strong for arbitrary models, but is almost optimally weak for models that obey chiral symmetry. This use of chiral symmetry, which mandates that multi-pion-exchange potentials (including 3NF) get progressively weaker, was first proposed by Gerry Brown and collaborators[27]. Without this constraint the nuclear force would be intractably complicated.

The second major influence of chiral symmetry was its incorporation as an essential ingredient in QCD. It is difficult now to imagine nuclear physics without QCD. Everyone “talks” QCD; “finding” quarks in nuclei has become our search for the Holy Grail of medieval legend. Unfortunately, this has had minimal impact on our understanding of low-energy nuclear physics. The problem is that quarks (constituents) and gluons (the binding mechanism) don’t exist as free particles. Moreover, it only requires collisions between nuclei with a few MeV of energy to experimentally demonstrate that nucleons are the most important degree of freedom in a nucleus (at any modest energy scale). More energy (~ 150 MeV) shows that pions are the next most important, etc. How then can we interpolate between these two descriptions of nuclei, which appear so fundamentally different, even contradictory?

I have spent a lot of time lately thinking about these issues, and talking to

experts in the field, who hold widely divergent views. My own view is summarized as: degrees of freedom in physics are a choice, not an obligation. If one chooses to describe a system with “good” d.o.f., the subsequent description will be economical and “clean”. Choosing “bad” ones will be uneconomical and “ugly”. Neither set is correct or incorrect. A possible way out of this dilemma has been afforded by Weinberg[28] (and many others, including Gasser and Leutwyler[8]). QCD can be “mapped” onto a set of “effective” d.o.f., which might, for example, include nucleons, pions, and Δ isobars. A field theory for the interactions between them can then be constructed that manifests the correct (broken) chiral symmetry, and potentials can be constructed using this interaction. OPEP is an immediate and not unexpected result. This scheme is called Chiral Perturbation Theory, and is a surrogate for the underlying QCD.

Unfortunately, this scenario is not expected to work well above an energy scale, Λ , on the order of 1 GeV. Above that energy there should be a gradual transition to another (and more economical) description. Consequently, all short-range (very massive, $M \gtrsim \Lambda$) objects except for the nucleon and possibly the Δ are considered more appropriate to the underlying QCD and are “frozen out”. Because massive particles cannot propagate very far, their effect is replaced by δ -functions with arbitrary coefficients.

My first impression of χ PT was that it had a good “feel” to it, because it combined principle and phenomenology in a way that nuclear physicists have always done (or tried to do). Phenomenology is required because the δ -function coefficients that subsume the influence of hard (short-range) QCD effects must be fit to data. This scheme is exemplified by the meson sector, where more than 10 observables are neatly fit by only two parameters[29]. There have been successes in the nucleon sector, as well. It should be noted that this field theory is not renormalizable in the old sense of that term, but this is not a problem, as we were told by Peter Lepage[30].

To lowest order, the NN potential is given by OPEP plus δ -functions that represent ρ -, ω -, ... exchanges. Recall from quantum mechanics that the δ -function potential gives a sensible constant Born approximation scattering amplitude, but diverges in higher orders. There are two ways to handle the divergences. One is to fatten the δ -function (i.e., make it have finite extent[31]) and the other is to renormalize the perturbation expansion in some way. Although the latter approach would be the cleaner alternative, I have no idea how to do it in an economical way. Our traditional nuclear physics methods are economical, if they are nothing else. For example, we “renormalize” by using form factor cutoffs. Finding a way to resolve this technical problem, or

showing that we cannot do it at all, is in my view extremely important.

- In my opinion one of the major theoretical problems in our field is to determine whether χ PT can provide a sound and economical framework for understanding few-nucleon systems.

ARE NUCLEI SOFT AND NATURAL?

Not to be confused with an advertisement for shampoo, a positive answer to the question could make a profound impact on nuclear physics. The question is another way of phrasing the “bullet” above. For any dynamical scheme to work effectively, it needs to converge rapidly. This is especially true for schemes based on χ PT, since each order of perturbation theory introduces more and more unknown constants, which can only be determined from experiments. Clearly, if convergence is not rapid there is little hope for this approach.

In nuclear physics, there are basically two ways to proceed with dynamics. We can either directly calculate amplitudes from the underlying fields (the “QHD” approach[32]), or one can calculate a potential from these ingredients and calculate amplitudes using the potentials. The potential scheme is the one we are most familiar with in treating FNS. It is rather efficient because, after a strong, hard interaction between two nucleons and before another collision occurs, the nucleons can “coast”. In weakly bound systems (or in scattering) the Green’s functions responsible for “coasting” can be nearly singular, and this infrared singularity means that successive collisions are (roughly) as important as the first one. Consequently, a very complicated amplitude can be obtained (in practice) from a much simpler (in principle) object: the potential (providing that we can calculate the latter).

As stated above, the dynamics of χ PT is expressed as a series in dimensionless variables[33,34]. The two mass scales of the problem are Λ (~ 1 GeV), the QCD large-mass scale, and f_π (~ 93 MeV), the pion decay constant. For energy or momentum scales less than (or comparable to) Λ , nucleons, pions, etc., should be a good set of d.o.f. to use. We heard an entire session of talks that suggests that this is reasonable. At a recent CEBAF workshop we learned from the practitioners that in electromagnetic interactions with nuclei for increasing momentum transfers, $q > \Lambda$, any and all “improvements” in the physics give roughly the same effect, implying to me that in this regime

mesons and nucleons may be increasingly poor choices for d.o.f.

The Lagrangian of χ PT can be written as a series in the various (unspecified) fields with these length scales and dimensionless coefficients[34]:

$$\mathcal{L} \sim \sum_{i,j} c_{ij} \left[\frac{()}{\Lambda} \right]^i \left[\frac{()}{f_\pi} \right]^j . \quad (3)$$

If nuclei are “natural”, then $c_{ij} = \text{Order}(1)$, and if nuclei are “soft”, the series converges in some sense. As an example[31] of “natural”, strong interaction coupling constants should have the form

$$\frac{G}{\Lambda} \sim \frac{1}{f_\pi} , \quad (4)$$

or $G \sim 10$. Thus, large strong-interaction coupling constants are natural.

Following Weinberg[28] and van Kolck[31], we can use power counting in Λ^{-n} to estimate the size of various contributions to potentials, which leads to

$$\langle V_{NN} \rangle > \langle V_{3N} \rangle > \langle V_{4N} \rangle \cdots . \quad (5)$$

This result, that successive multi-nucleon forces get progressively weaker, had already been anticipated at the beginning of the talk. In the triton, 3NF cannot contribute more than $\sim 2\%$ of NN forces, while 4NF appear not to be needed at all in the α -particle or ${}^6\text{Li}$. Recently, van Kolck[31] has used the same ideas to demonstrate that charge dependence in the NN force is bigger than normal charge-symmetry breaking (n-n vs. p-p), which is bigger than CSB in the n-p system:

$$\langle V_{CD} \rangle > \langle V_{CSB}(T=1) \rangle > \langle V_{CSB}(\text{n-p}) \rangle , \quad (6)$$

which holds experimentally, but was always something of a mystery to me.

The approach to nuclear dynamics that we have described here is very unusual, and it remains to be seen how well it works in practice. I believe that it has promise beyond the dominance proofs in Eqns. (5) and (6). I will finish by quoting Steve Weinberg[35]: “...the chiral Lagrangian approach turns out to justify approximations (such as assuming the dominance of two-body interactions) that have been used for many years by nuclear physicists...”. Hopefully, there will be more successes.

ACKNOWLEDGEMENTS

This work was performed under the auspices of the U. S. Department of Energy.

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